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Capt. George Richard Stevens, F.R.G.S., 12 Abingdon Villas, Kensington, W.; and

John Fletcher Moulton, M.A., of Christ's College, Cambridge, were balloted for and duly elected Fellows of the Society.

Mr. John Williams, Assistant Secretary of the Society, died on the 3rd of December 1874, in his 78th year. The following Resolution was passed at the Meeting of the Council:—

Resolved that—

The Secretaries be requested to communicate to the Eldest Son of the late Mr. John Williams the deep sympathy felt by the Council in the bereavement which he and the other members of his family have just experienced; and at the same time to express the high appreciation of the Council of the moral worth and zealous services of their late Assistant Secretary in the discharge of his duties during the long period of twenty-eight years for which he was connected with the Society.

On certain Phenomena presented by the Shadows of the Satellites of Jupiter while in Transit; and on a possible Method of deducing the Depth of the Planet's Atmosphere from such Observations. By C. E. Burton, Esq.

Several observers of eminence have recorded, on different occasions, various anomalous appearances presented by the shadows of the satellites of *Jupiter* when in transit over his disk. From these records I have selected two, which describe appearances seemingly not very diverse in character from those hereafter to be mentioned.

The first of these is due to the late Sir J. South, who, with two telescopes of different size and power, detected on one occasion that each of two satellite shadows on the planet at the same time was attended by a faint duplicate of itself. Observation cited by the Rev. T. W. Webb, in his work *Celestial Objects*, p. 140, 2nd Edition.

In 1850 Mr. Lassell noted that the fourth satellite appeared markedly smaller than its shadow, and that the shadow was sen-

sibly round. The observation was made with Mr. Lassell's 9-inch reflector of 9-feet focal length, and the planet was at the time near opposition. The connection of the position of the planet relatively to the earth, with the observed form of the satellite shadow, will become more evident in the latter portion of the present paper, supposing that the hypothesis therein advanced should prove worthy of attention after due examination of the observations next to be detailed.

September 19, 1870, $15^h\ 25^m$ to $16^h\ 55^m$, G. M. T. Elongation of Earth, seen from *Jupiter* = 12° 58′.

Observing *Jupiter* with an 8-inch silver-on-glass reflector of 6-feet focus, a power of 520 measured diameters being in use, with which the planet and satellites were generally beautifully defined, I perceived that the shadow of the third satellite, which had entered the disk at the earlier epoch, was not round, but decidedly elongated in the direction of the parallel upon which it fell. The watch was kept up for an hour and a half, during which period the form of the shadow-spot underwent scarcely any sensible change. This form was, roughly speaking, that of a lozenge, the proportion of the principal diameters being as 1*. It fell on a narrow dark band. The satellites I, II, and III were for a time seen as neat, sharp, *round* disks, the last-mentioned displaying a dusky shading of irregular depth on its northern hemisphere. IV was irregular in outline.

October 3, 1870. Elongation of Earth, as seen from *Jupiter* = 11° 1′.

On this occasion the shadow of II on the planet was observed to be sensibly round, and blackest when definition was most steady. III and IV, which were close together, appeared very dissimilar, III being white, IV dusky, and, I believe, not round, but polygonal. II appeared beautifully round at the same time. Instrument, and, apparently the power used, identical with those employed on September 19.

November 10, 1870. Elongation of Earth, seen from *Jupiter* = 5° 19′.

The shadow of I appeared round, which form was maintained up to internal contact with the limb; it was afterwards seen bisected on the limb, and as a gradually diminishing segment of a circle. The shadow fell, I believe, on the equatoreal dusky region, but the obliquity of the line of sight was small.

Instrument used, the 8-in. reflector; but the power is not stated.

January 1, 1872. Elongation of Earth, as seen from *Jupiter*, or obliquity of line of sight = 3° 02′.

The shadow of the second satellite, which had accomplished two thirds of its passage, was several times seen as elongated in the

* So in M.S., but ?. The author, being with the Transit Expedition at Rodriguez, has not corrected the proofs.—Ed.

preceding and following direction, the longer axis being slightly oblique to the parallel on which it fell. It was situate just within the southern edge of the great equatoreal dusky belt, which was ochreish in tint and mottled with white. The planet was fairly defined with power 228 on the 7-in. speculum in use at the time. III and IV were occasionally very well shown with power 520. The observation was made under somewhat unfavourable circumstances, which were not, however, sufficiently adverse to warrant its exclusion from the series. The wind was very high, and the instrument occasionally considerably shaken.

January 8, 1872. Obliquity of line of sight = 1° 33′.

On this occasion the shadow-spot of II was seen for an instant as a round dot with the 7·2-in. reflector, armed with a power of 270 or 228, which power showed an immense amount of minute detail on the planet.

The character of the surface in the neighbourhood of the shadow-spot is not recorded, but it fell on the equatoreal dusky region. The observation was considered good, though brief. The planet is now near opposition.

March 6, 1872. Obliquity of line of sight = 9° 05′.

The shadow of IV in transit was observed under very favourable circumstances as to definition, etc., and three sketches of the form presented by it at $9^h\ 30^m$, $9^h\ 55^m$, and $10^h\ 0^m$, G.M.T. were made. The form recorded at the latest epoch was confirmed by another and independent observer using the same instrument. The shadow-spot was unmistakably elongated, in the direction of the belts, and was situated on a dusky band of irregular depth of tint.

The extreme diameters of the spot were variously estimated to be in the ratio 5 : 4 and 4 : 3. At $9^h\ 35^m$ it was noted that the north and south boundaries of the spot were sharply defined, while the preceding and following extremities were ill-defined. The disks of I, II, and IV were at times well shown, especially the two former. Power 408 on 7·2-in. reflector.

The form of the spot was carefully compared with that of the satellite's disks, which were beautifully round, and very sharp at times, and turning the head through a considerable angle from the vertical laterally, in order to guard against possible cylindricity of the crystalline lens. The observation was one of the most complete and satisfactory of the whole series.

It is to be observed that the obliquity of the line of sight was considerable, and that the shadow fell on a dusky region.

April 24, 1872. Obliquity of line of sight = 9° 27′.

The shadow of the first satellite was seen on a dark patch of belt 4 (Mr. Gledhill's notation). It was not round, and the

major diameter was considered to be in the direction of the belt. The ratio of the extreme diameters was considered to be 1·2 or 1·3 to 1. The diameter of the shadow-spot was nearly equal to the diameter of the satellite itself, or perhaps more nearly to that of II. A penumbra was suspected at times with power 250.

The observation was considered good.

We note here that the obliquity of the line of sight is considerable; that the shadow was thrown on a dusky belt; and that the certain part of the observation is the elongation of the shadow-spot.

The instrument used was an exquisitely defining object-glass by Alvan Clark, of $7\frac{1}{2}$-inches aperture, formerly belonging to Mr. Dawes, armed with powers of 250, and, I believe, 400, and equatoreally mounted in the observatory of W. Erck, Esq., Sherrington, Bray, Co. Dublin.

September 19, 1872, $16^h\ 40^m$ to $17^h\ 7^m$ G.M.T. Obliquity of line of sight = 6° 24′.

The shadow of the *third* satellite in transit was observed not to be circular in form At first it was seen to be winged both on the preceding and following sides; a little later the wing on the preceding side was the only one visible, and at $17^h\ 7^m$ the shadow-spot had resumed its doubly winged aspect, at which time the satellite (III) had entered on the planet to the extent of half its diameter.

About $16^h\ 55^m$ the north-preceding portion of the shadow had come into apparent contact with a bright spot, within the equatoreal dusky belt, which the shadow was traversing, and at that time the wing on the side of the contact appeared to be deviated slightly upwards, *i.e.*, southwards and away from the bright patch.

When the elongation of the spot was greatest the ratio of its extreme diameters was estimated carefully as 5 : 4.

I was compelled to defer the completion of the record until 7^h of the 20th September, when I added from recollection that the shadow appeared very slightly larger than the transiting satellite, which was perfectly round.

The obliquity of the line of sight was a medium one, and the shadow fell on a dusky ground. The instrument employed was a silver-on-glass reflector of 12-in. aperture, armed with a power of 408.

January 16, 1873. Obliquity of line of sight = 5° 32′.

The shadow of I, which probably fell within the limits of the equatoreal dusky belt, was carefully scrutinised without any defect of roundness being detected. A suspicion that there was a slight penumbra surrounding the shadow was entertained, but definition was at that time slightly diffused. The shadow was

extremely minute, appearing perhaps smaller than II of the planet. Satellite I was on the planet, and invisible.

The record was not made until the 20th, when, however, my recollection was still perfectly clear.

The observation was considered good; and we have to note that the obliquity of the line of sight was below its mean value, and that the shadow fell on a portion of the surface which was neither bright nor dark.

Instrument used, the 12-in. reflector, power 400.

February 3, 1873. Obliquity of line of sight = 2° 19″.

The shadow of I was scrutinised when in transit with the 12-inch reflector, under powers of 238 and 409 diameters. No defect of roundness was discoverable, and its diameter appeared to be little more than half that of III, which was also sensibly circular.

Definition generally fluttering and furry, but quiet for a few seconds together at distant intervals. Observation considered good. The shadow fell on the equatoreal belt, which was at that time somewhat bright. The obliquity of the line of sight was small.

February 20, 1873. Obliquity of line of sight = 1° 01′.

The shadow of II in mid-transit was observed to be sensibly round, no irregularity of form being discoverable under powers of 238, 409, and 514; even, on comparing it with II, which was on the planet, close to the limb and appeared slightly larger than its shadow, the latter falling during its passage on different parts of the great dusky equatoreal belt and just within its northern limit.

Definition, at first fluttering, and only at moments distinct. became continuously good at the times of emersion of the satellite, With power 514 the *third* satellite appeared unequally shaded.

February 27, 1873. Obliquity of line of sight = 2° 41′.

The shadow of II was observed not to be perfectly round, like the satellite itself, but irregular in outline, and with power 409 it was less sharply defined than the satellites. I thought that there were some indications of a penumbra. The shadow-spot was situate, I believe, upon the dusky equatoreal zone.

The observation was considered good, and it is to be noted that the obliquity of the line of sight was small.

March 12, 1873. Obliquity of the line of sight = 5° 10′.

The shadow of I being in transit over the equatoreal dusky region, was scrutinised with an equatoreal reflector of 8-inches aperture, and power 280, without my being able to detect with certainty any difference between it and the second satellite in respect of form.

A sketch of the form of a shading on the disk of I, detected while it was in transit, is appended. Fig. 1. *a*.

Fig. 1. a.

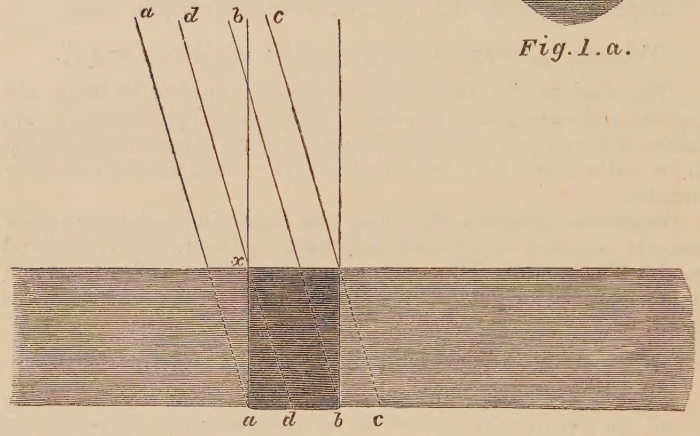


Fig. 1.

Fig. 2.

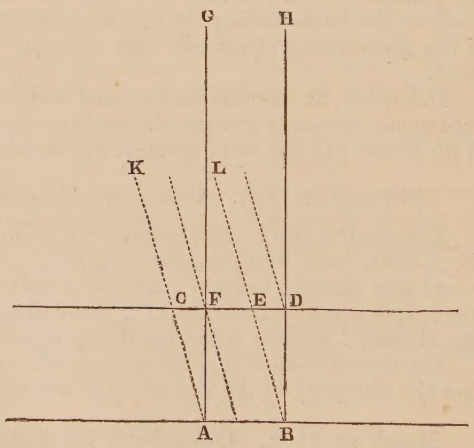


Fig. 3.

The observation was considered good. The obliquity of the line of sight was approaching its mean value; but it is to be observed that the shadow fell on a portion of the surface of the

dusky equatoreal belt, which was at this time brighter than the medium tint.

April 8, 1873. Obliquity of the line of sight = 9° 03′.

The shadow of III was observed in transit, and a sketch made about $8^h\ 15^m$. It was elongated in the direction of its motion, but not much so, the projection on the preceding side being visible only when definition was at its best, under power 514 on the 12-inch reflector. The shadow had completed about four-fifths of its passage, and fell very near to the northern edge of the dark belt No. 3 Gledhill. The shadow was perfectly black over the greater part of its area. There was a slight protuberance on the side, much smaller than that on the opposite portion, which latter faded rapidly into the background of the dusky belt.

At times I failed to see the above-mentioned wings of the shadow, especially that on the following side; but these variations appeared to be unconnected with variations in the tint of the regions traversed by the shadow, and to be due solely to the occasional undulations of the air which at intervals distorted the disks of the satellites. No trace of true penumbra was seen, the north and south limbs of the shadow appearing free from any indistinctness. The two principal diameters were estimated to be in the ratio of 5 : 4 at the utmost, and the shadow was considered to be very slightly smaller than III outside the disk, which was exquisitely defined and unequally shaded.

The observation was given a high weight, the circumstances being unusually good. The line of sight had attained a considerable obliquity, and the region traversed by the shadow was of medium tint.

April 12, 1873, $8^h\ 45^m$ to $8^h\ 55^m$, G.M.T. Obliquity of line of sight = 9° 25′.

The shadow of IV was observed to be elongated in the preceding and following direction, and the ratio of the axes was estimated at 5 : 4 or 4 : 3. It was not perfectly sharp, and appeared slightly larger than IV itself outside the disk of its primary. IV itself appeared to be somewhat softly defined, and slightly oval in the same direction as its shadow, I, which was near it, being perfectly sharp and round, under the same power, and at the same time. The shadow fell on a slightly shaded space within the bright band separating the dusky belts Nos. 2 and 3 in Mr. Gledhill's nomenclature; apparently a knot or projection from the lower or northern boundary of No. 3.

About $9^h\ 25^m$ IV appeared nearly, but not quite circular, the deficiency conceived to exist being situate on the preceding or north-preceding portion of the satellite's disk, and very small in amount. The shadow still elongated, and to about the same extent as at first. At $9^h\ 55^m$ the shadow was found to be on a narrow oblique dusky belt connecting the dusky belts 2 and 3

mentioned above, and still elliptic in form, in the same direction as before, the ratio of the axes being now about 5 : 4. Its diameter was very nearly equal to that of IV. The observation, which was made with the 12-inch reflector, was given full weight. The power used was not recorded, but was in all probability either 409 or 514, as these powers were almost invariably employed for scrutinising the satellites and their shadows.

The obliquity of the line of sight was still considerable, and the shadow traversed a dusky region.

May 6, 1873. $10^h \pm$ G.M.T. Obliquity of line of sight = 10° 45′.

The shadow of I a little past mid-transit appeared elongated, the ratio of the axes being by estimation as great as 3 : 2, and the minor axis very slightly less than that of I outside the disk of the planet. This was the result of a tolerably good view lasting only a few seconds. The shadow did not appear black, definition being soft. It fell in a very dark patch near to the northern edge of the equatoreal dusky belt.

Next day I recorded that III appeared elongated in the same direction as the shadow, and that I, as I thought, did *not* present this aspect.

On this occasion the circumstances were rather unfavourable, and less weight is therefore to be attached to the observations made. The obliquity of the line of sight was considerable, and the elongation of the shadow seems to have been great.

December 19, 1873. Obliquity of line of sight = 10° 25′.

The shadow of IV in transit was observed when it had completed about three-fourths of its passage, and was at times well seen with a power of 400 on the 12-inch reflector. It did not appear to be circular, my impression being that it was slightly lozenge-shaped, or irregularly elliptical, and that the ratio of the extreme diameters was 6 : 5. The form of the shadow certainly seemed to differ markedly from that of III, which was shown by the same power as a neat circular disk. IV I thought to be elliptic, the major axis lying nearly in the preceding and following direction. The major axis of the shadow-spot was considered to be parallel to the belts.

It did not appear perfectly black, and its diameter considerably exceeded that of IV, which was some distance from the planet.

The shadow fell upon the northern polar shading, which was nearly uniform in tint and light grey tinged with blue. The shadow was not so sharply defined as the limb of the planet, or the disk of III.

I turned my head laterally while observing, through 30° about the emergent pencil as axis, without perceiving any change in the form of the shadow. There was no distortion of the disks of the satellites.

The colour of the equatoreal region, a faint ochreish yellow, contrasted finely with the bluish grey of the north polar cap. The observation was satisfactory though brief, and the comparisons are considered reliable, but the deviation of the shadow-spot from a circular form was so slight that I could not assure myself of its precise configuration. The obliquity of the line of sight is here large; but it is to be noted that the region traversed by the shadow-spot, though dusky, probably differs in its nature from the equatoreal zones within which the paths of the shadow were on former occasions comprised.

May 19, 1874. Obliquity of line of sight = 9° 50′.

The shadow of IV being on the disk of *Jupiter* at G.M.T. was carefully scrutinised by Mr. Erck and myself with Mr. Erck's $7\frac{1}{2}$-inch Equatoreal armed with a power of 250, and compared as to form with the satellite's disks, which last were perfectly free from distortion when best seen, which was not the case with the shadow. The shadow was strongly elliptical, and the ratio of the axes was estimated to be 3 : 2.

The minor axis did not, I believe, differ much in length from the diameter of the satellite (IV).

The major axis was inclined to a parallel of Jovian latitude by 15° or 20°. Both the observers agreed as to this point.

The shadow was situate on the grey northern cap during the whole of the observation.

The extremities of the major axis appeared to be much more nebulous than any other portion of the outline of the shadow.

The dusky equatoreal zone had a strong coppery tinge, and the limb of the planet furthest from the Sun was decidedly ill-defined, the terminator on the E. side being within the Jovian hemisphere next the Earth.

The observation was exceptionally good, and possesses the additional confirmation of the independent testimony of an experienced observer.

The obliquity of the line of sight was considerable, and the region on which the shadow fell was dusky in tint.

At the commencement of the next division of the subject, namely, the connection of the foregoing observations with a hypothesis put forward as early as April 1871, to explain the appearances detected, I will call attention to the apparent dependence of their variations upon the relative positions of the Sun, the Earth, and *Jupiter*; the elongated form of the shadow-spots being visible, with one apparently unimportant exception, only when *Jupiter* is near quadrature, or, in other words, when the line joining *Jupiter* and the Sun attains its maximum inclination to the line connecting *Jupiter* and the Earth.

This dependence will become more evident if the recorded observations are arranged in a tabular form in juxtaposition

with the corresponding values of the angle included between the visual direction and the axis of the shadow cone or *Jupiter's* radius vector for the epoch of observation, which angle is that expressing the obliquity of the line of sight. See headings to observations in the former part of this paper.

Tabular Summary of Observations.

TABLE I.

Date.	Sat.	Form of Sh.	Obliquity. ° ′	Nature of Surface.	Instrument.	Weight.
1870 Sept. 19	III	Elong.	12 58	Dark belt	8-in. reflector	Obs. good
Oct. 3	II	Round	11 01	Bright belt	„ „	„ „
Nov. 10	I	Round	5 19	Dusky eq. belt	„ „	„ „
1872 Jan. 1	II	Elong.	3 02	Equ. dark belt	7-in. reflector	Obs. defective
Jan. 8	II	Round	1 33	Probly. as last	„ „	„ good
Mar. 6	IV	Elong.	9 05	Equ. dark belt	„ „	„ „
Apr. 24	I	Elong.	9 27	Equat. dark	7½-in. object-glass by Alvan Clark	„ „
Sept. 19	III	Elong.	6 24	Equat. dusky	12-in. reflector	„ „
1873 Jan. 16	I	Round	5 32	Probably equa. dusky belt	„ „	„ „
Feb. 3	I	Round	2 19	Probably same region	„ „	„ „
Feb. 20	II	Round	1 01	Same region	„ „	„ „
Feb. 27	II	Irreg. not Elong.	2 41	„ probably	„ „	„ „
Mar. 12	I	Round	5 10	Equat. d. belt	8-in. reflector	„ „
Apr. 8	III	Elong.	9 03	„	12-in. „	„ „
Apr. 12	IV	Elong.	9 25	Shaded space in bright belt	„	„ „
May 6	I	Elong.	10 45	Very dark part of eqtl. d. belt	„ „	Pretty good
Dec. 19	IV	Elong.	10 25	Grey n. cap.	„ „	Good
1874 May 19	IV	Elong.	9 50	„ „	7½-in. Alvan Clark	„

On comparing the forms of the shadows with the corresponding obliquities, it will be at once evident that a small obliquity is, with one exception, and that the most doubtful of the series of observations (Table I, 1872, January 1), accompanied by a circular form of shadow.

The converse cannot be so broadly stated, two exceptions, observed under favourable circumstances, presenting themselves (Table I, 1870, October 3, and 1870, November 10?), differing from that just mentioned in that they are readily explicable on the hypo-

thesis put forward in the present paper, which assumes that the variations of brightness on the disk of *Jupiter* are due to the greater or less accumulation of masses of cloud, separately invisible and distributed through an extremely transparent atmosphere of enormous depth.

If a shadow of a satellite fall upon such an atmosphere, all the cumulus clouds within the shadow will be rendered dark and invisible.

If the shadow-spot be viewed when the earth is near the axis of the shadow cone, the part of the atmosphere in shadow will appear as a tolerably well-defined dark, almost circular spot.

On the other hand, if the shadow be viewed in a direction somewhat inclined to its axis, along the line *aa* or *cc* (Fig. 1) the clouds in the immediate neighbourhood of either of those lines will all be fully illuminated, and the brightness of the surface at that point will be sensibly the same with that of the surface outside those lines.

Again, the space between *bb* and *dd* falls within the unilluminated region, and will therefore return no light to the eye. This portion will therefore appear sensibly black.

And it is evident, from a consideration of the figure, that the darkness will gradually shade off from *dd* to *aa*, and from *bb* to *cc*. Now this is precisely the appearance presented when the observer was in a situation sufficiently removed from the axis of the shadow, and when the portion of *Jupiter* on which the shadow fell was dusky; in other words, when the clouds were sufficiently scattered to allow of our seeing to the lower strata of the immense atmosphere of *Jupiter*.

The form of the darkest portion of the shadow-spot seen from a distance will manifestly be lenticular, being contained between the projected outlines of the circular segments of the upper and lower sections of the shadow at the level *x* and at the level *ab*.

The apparent form of the shadow seen by an observer so placed will closely resemble that shown in Fig. 2, where the line *ac* indicates the plane of the section of sketch Fig. 1.

In the foregoing paragraphs it has been assumed, in order to avoid unnecesary complication, that the penumbræ of the shadows due to the sensible angular diameter of the Sun are imperceptible.

Geometrically this can never be the case, but the breadth of the penumbra is so small that its only visible effect would be to render the upper and lower edges of the dark spot (Fig. 2) very slightly less defined.

To this hypothesis it may be objected, that it is not immediately evident why it should be preferred to another which seems to have certain points in its favour, namely, that which supposes *Jupiter* to shine merely as a phosphorescent body, by the solar light.

This hypothesis demands the additional assumption that the surface does not acquire or lose its brilliancy at once when the Sun's light falls on it or is withdrawn.

By making appropriate suppositions as to the retentiveness of

light on the part of the atmosphere, it is evident that the aspect of the shadow-spot may be supposed to vary from that of a sharply defined dark patch corresponding in form to the section of the shadow cone at the level of the obscured region, through the intermediate stages of a continually diminishing black patch of lenticular form to the final aspect of a barely discernible shade travelling over the surface of the planet, the wings on the preceding and following sides, where the light is not acting fully, having coalesced.

This hypothesis fails to meet the facts in one very important respect, for, according to it, the winged aspect of the shadow ought to be presented at every transit, irrespective of the position of the observer. This is not the case according to the observations cited above, and we may therefore consider the original supposition as the more probable one.

We are by it led to the conclusion, that the degree of elongation visible will depend on these three conditions: the obliquity of the line of sight, the depth of the clouds stratum, and the degree of aggregation of the floating cumuli. This is at once evident on inspection of Fig. 1.

The mode of deducing the depth of the atmosphere from the data now in our possession is perhaps most readily shown by an outline reproduction of Fig. 1 in Fig. 3, where CD is the upper limit of the cloud stratum, AB the lower limit of visible shadow penetration; and where GA and HB are the bounding lines of the shadow cone lying in the plane bisecting the shadow-spot at its longest diameter, and KA, EB, LD are portions of lines drawn in the same plane from the observer's eye to the points A, B, D, and are sensibly parallel to one another, as also are the lines FD, AB; GN, HB respectively.

Therefore CE=AB=FD=diameter of shadow cone=minor diameter of shadow-spot.

CF is therefore the difference between the greatest and least diameters of the shadow-spot.

The ratio CD : FD being known from the observations, and also the approximate length of FD in miles as well as the angle CAF or the obliquity, we have at once

$$\text{Depth AF in miles} = \text{CF} \cdot \cot \text{CAF}$$
$$= \frac{\text{CD}-\text{FD}}{\text{FD}} \times \text{No. miles in FD} \times \cot \text{CAF}.$$

Applying this formula to the observations, we deduce the following results, embodied in Table II, in which the depths of shadow penetration vary between 3,200 and 9,200 miles.

TABLE II.

Date.	Sat.	Diam of Sh. assumed. Miles.	$\frac{CD-FD}{FD}$	Depth deduced.	Weight attributed.	Remarks.
1870 Sept. 19	III	3300	0·4	5700	Obs. good	Elongation deduced from sketch
1872 March 6	IV	2800	0·3	5200	„ „	
April 24	I	2100	0·25	3200	„ „	
Sept. 29	III	3500	0·25	7800	„ „	
1873 April 8	III	3300	0·2	4000	„ „	
April 12	IV	2800	0·3	4200	„ „	
May 6	I	2100	0·5	5500	„ tolerable	Air unsteady
Dec. 19	IV	3500	0·2	3800	„ good	Amount of elongation doubtful
1874 May 12	IV	3200	0·5	9200	„ very good	Incidence very oblique.

The exceptionally large result just quoted is apparently due to the low angle of incidence at which the shadow met the surface of the cloud region; falling, as it did, very near to the pole of the planet, and therefore probably traversing a much greater extent of atmosphere than if it had been thrown on the central regions of the planet.

The numbers given in Table II, column 5, are probably less than they ought to be; the ratio $\frac{CD}{FD}$ having been estimated too low on account of the gradual fading of the elongated shadow-spot into the tint of the surface in the immediate neighbourhood of the extremities of its major axis; which renders it impossible to say definitely where it terminates.

The estimation of the ratio is injuriously affected also by the extreme minuteness of the angle under which the spot is seen, which probably never exceeds $1''\cdot75$. In determining the forms of such minute objects the eye is probably more trustworthy than the micrometer, that is, if it has received a careful training in the work required to be done.

It is plain that the well-known suggestion which was made soon after the discovery of the low mean specific gravity of *Jupiter*, that within an atmosphere of great depth there existed a nucleus approaching in density to that of the interior planets, is, in part, an anticipation of that put forward in the present memoir, and, by its reasonableness, has commended itself to many writers on planetary physics; but the additions now made to it, especially the assumption of the transparency of the atmosphere, perfect enough to permit us to trace a shadow through it for nearly 10,000 miles, is sufficiently startling to induce great hesitation in accepting the modified hypothesis. One agreement between the results of this hypothesis and observation may be pointed out.

(1.) It has long been known that the limb of *Jupiter* is much less brilliant than the centre of the disk, and that the brightness rapidly increases with the distance from the limb:

(2.) also that the apparent orbital motions of the satellites when undergoing or emerging from occultation are unaffected by refraction.

(3.) We have lately been made aware that the atmosphere of *Jupiter* probably contains a considerable proportion of aqueous vapour mingled with other vapours as yet unknown.

(2) and (3) taken together render it likely that the selective absorption observed takes place below the upper cloud stratum, and amongst the separate clouds floating in the atmosphere, somewhat in the same manner as the selective absorption producing the Fraunhofer lines appears to have its seat among the granules of the solar photosphere, as far as many of those lines are concerned.

The Jovian clouds may rise almost to the upper limits of the sensibly refracting atmosphere, and may be somewhat scattered, so that in the neighbourhood of the limb the visual ray may here and there pass amongst them freely.

The number of such openings will evidently increase rapidly toward the limb, which will consequently diminish in brightness with equal rapidity, the dark background of space becoming visible in an augmented ratio.

The solar atmospheric surroundings present the only known instance of a gaseous envelope of a depth and transparency equalling or rather far surpassing those attributed in this paper to the atmosphere of *Jupiter*.

But the temperatures at which the respective atmospheres are maintained in the case of the two bodies probably differ enormously, and the density of the lower stratum of the Jovian atmosphere must consequently be extremely great, if its depth approaches even the least of those assigned to it in Table II.

This probability constitutes a difficulty, and a great one, in the way of accepting Table II, col. 5; which further observation and experiment may, it is hoped, remove.

One argument in favour of the supposed constitution may, perhaps, be drawn from certain observations made under favourable circumstances by Messrs. Schwabe and Jacob many years since, and more recently by the Earl of Rosse, to the effect that the cloudy masses of the Jovian atmosphere are, occasionally at least, broken up into fine filaments or elongated particles, especially in the neighbourhood of the equator. I may, perhaps, be permitted to add, that I have repeatedly received a similar impression when working with different instruments of apertures (effective) varying from 7 to 12 inches, and using high powers; the equatoreal region alone presenting this appearance. The breadth of the fine streaks referred to in the last sentence must have been less than $0''{\cdot}25$.

Here I leave the subject in the hands of far abler observers

than myself, by whose verdict the question will ultimately be decided, and regretting that my own opportunities of collecting evidence have been so few and far between.

Loughlingstown, Co. Dublin.

Letter from an Officer in the Merchant Navy on the Application of Corrections for Change of Temperature to the Rates of two Chronometers during a voyage from Liverpool to Calcutta.

(*Communicated by Mr. Hartnup.*)

During my last voyage from Liverpool to Calcutta, on board the ship "Tenasserim," I had an opportunity, through the kindness of the Commander, Captain T. C. Potts, of testing the practical value of your method of finding the rate of a chronometer at sea by the application of corrections due to change of temperature.

In 1873 I obtained a copy of your Report to the Marine Committee of the Mersey Docks and Harbour Board for the preceding year, in which you have given examples of the method of making the necessary calculations, and I have followed the instructions there given. The two chronometers used on the voyage were named "J. Basnett & Son, No. 713," and "Thomas Blundel, No. 209." The data from which I made my calculations I obtained from two certificates given by you at the Bidston Observatory in September 1871, from which I obtained the following results:—

No. 209.

Rate in 55° = $-5^{\circ}\cdot58\ r$
„ 70° = $-4^{\circ}\cdot00\ r'$
„ 85° = $-2^{\circ}\cdot70\ r''$

$r - r' = -1^{s}\cdot58 = d$
$r' - r'' = -1^{s}\cdot30 = d'$
$d - d' = -0^{s}\cdot28$
$d + d' = -2^{s}\cdot88;$

$$C = \frac{2(d-d')}{30^2} = \frac{-0\cdot56}{900} = -\cdot00062;$$

$$T - 70 = \frac{d+d'}{C \times 60} = \frac{-2\cdot88}{-0\cdot0372} = +77\cdot4;$$

$$T = 70 + 77\cdot4 = 147\cdot4;$$

$$R = r' - (T-70)\frac{d+d'}{60} = -4\cdot00 + 77\cdot4 \times \cdot048 = -0\cdot28.$$

No. 713.

Rate in 55° = $-0^{s}\cdot72\ r$
„ 70° = $-0^{s}\cdot27\ r'$
„ 85° = $-1^{s}\cdot35\ r''$

$r - r' = -0^{s}\cdot45 = d$
$r' - r'' = +1^{s}\cdot08 = d'$
$d - d' = -1^{s}\cdot53$
$d + d' = +0^{s}\cdot63;$

$$C = \frac{2(d-d')}{30^2} = \frac{-3\cdot06}{900} = -\cdot0034;$$

$$T - 70 = \frac{d+d'}{C \times 60} = \frac{+0\cdot63}{-0\cdot204} = -3\cdot1;$$

$$T = 70 - 3\cdot1 = 66\cdot9;$$

$$R = r' - (T-70)\frac{d+d'}{60} = -0\cdot27 + 3\cdot1 \times 0\cdot0105 = -0\cdot24.$$

From the above calculations we have the following results :—

	Mean Daily Rate.					
	In 55°	In 70°	In 85°	C	T	R
	s	s	s		°	s
No. 209	−5·58	−4·00	−2·70	−0·00062	147·4	−0·28
No. 713	−0·72	−0·27	−1·35	−0·0034	66·9	−0·24

Let N = any number of degrees from T; then the rate at $T \pm N = R + C \times N^2$.

On the arrival of the "Tenasserim" at this port (Liverpool) in December 1873, the errors of both the chronometers were found by the time gun; and on her sailing on January 21, 1874, the errors were again obtained by the same means. The rates were found by dividing the difference of these errors by the number of days which elapsed between these two determinations. The following were the errors and rates as found, on January 21, 1874, by means of the time gun :—

No. 209.	No. 713.
Slow on G.M.T., $0^{h}\ 1^{m}\ 11^{s}\cdot0$.	Slow on G.M.T., $0^{h}\ 31^{m}\ 2^{s}\cdot0$.
Mean daily rate while in port losing, $1^{s}\cdot2$.	Losing $0^{s}\cdot8$.

The temperature to which the chronometers were exposed while in port was as nearly as I could ascertain 55° Fahrenheit; therefore in 55° the rate of No. 209 had between September 1871 and January 1874 altered from losing $5^{s}\cdot6$ to losing $1^{s}\cdot2$, and the rate of No. 713 had altered from losing $0^{s}\cdot7$ to losing $0^{s}\cdot8$. I find from your Report that the two factors C and T do not often alter, unless the chronometer is cleaned or repaired (which neither of them have been since September 1871). I have therefore assumed C and T to have remained constant, and I

have altered R in both chronometers agreeably to the above determinations of the ratio while in port. With these data, and following the rules laid down in your Report, I have calculated the following table of rates for every 5° of temperature (from 50°—90°), and using it, I have found the error in G.M.T. of each chronometer for every fifth day from January 21 to May 31, 1874:—

Table of Rates for every 5° of Temperature.

	50°	55°	60°	65°	70°	75°	80°	85°	90°	95°
	s	s	s	s	s	s	s	s	s	s
No. 209	−1·8	−1·2	−0·6	−0·1	+0·4	+0·9	+1·3	+1·7	+2·1	+2·2
No. 713	−1·3	−0·8	−0·5	−0·4	−0·4	−0·6	−0·9	−1·4	−2·1	−2·4

Captain Potts has been good enough to supply me with simultaneous readings of the faces of the two chronometers for every day of the voyage. The temperatures given are the means for each preceding five days obtained from the readings of a Board of Trade thermometer (which was kept in the chronometer-room) at 9 A.M. daily.

The results of my calculations, and the Observations of Captain Potts, are given in the Table on the following page.

On May 26, 1874, the errors of the two chronometers on G.M.T. were found to be:

	No. 209.	No. 713.
	h m s	h m s
By the Calcutta time gun	Fast 0 0 15·5	Slow 0 33 3·5
By calculation from rates corrected for change of temperature	Fast 0 0 19·0	Slow 0 33 12·0
Differences, or errors of longitude, by chronometer	0 0 3·5	0 0 8·5

You will see by the Table from the calculations for May 31, 1874, that, by correcting the rates for change of temperature, the two chronometers differed from each other in the G.M.T. shown by them by only *two seconds*; but by using the rates found in Liverpool in 55° temperature, they differed from each other to the large amount of *four minutes and fifty-two seconds* of time, or one degree and thirteen minutes of longitude.

Date. 1871.	Errors on Green. M.T. from calculations with Rates corrected for Change of Temperature. No. 209.	No. 713.	Difference between the two Chronometers from Calculation.	Comparison with each other.	Differences of Green. M.T. between the two Chronometers from Rates Corrected for Change of Temp.	Uncorrected for Change of Temp.	Mean Temp. Fahr.
	m s	m s	m s	m s	m s	m s	s
Jan. 21	−1 11·0	−31 2·0	29 51·0	29 51·0	0 0·0	0 0·0	00
26	−1 18·0	−31 7·0	29 49·0	29 44·5	0 4·5	0 4·5	53
31	−1 23·5	−31 10·5	29 47·0	29 44·0	0 3·0	0 3·0	56
					. . . .	. . . .	
Feb. 5	−1 26·0	−31 13·0	29 47·0	29 47·0	0 0·0	0 2·0	61
10	−1 26·0	−31 15·0	29 49·0	29 53·0	0 4·0	0 10·0	66
15	−1 21·5	−31 18·0	29 56·5	33 3·0	0 6·5	0 22 0	75
20	−1 15·0	−31 22·5	30 7·5	30 18·0	0 10·5	0 39·0	80
25	−1 9·0	−31 26·5	30 17·5	30 33·0	0 15·5	0 56·0	79
Mar. 2	−1 3·5	−31 30·0	30 26·5	30 44·0	0 17·5	1 9·0	77
7	−1 5·5	−31 32·5	30 27·0	30 45·0	0 18·0	1 12·0	62
12	−1 9·0	−31 35·0	30 26·0	30 46·5	0 20·5	1 15·5	59
17	−1 11·5	−31 37·5	30 26·0	30 47·5	0 21·5	1 18·5	61
22	−1 17·5	−31 41·5	30 24·0	30 43·0	0 19·0	1 16·0	55
27	−1 19·5	−31 44·0	30 24·5	30 41·5	0 17·0	1 17·5	62
Apr. 1	−1 18·0	−31 46·0	30 28·0	30 45·0	0 17·0	1 22·0	69
6	−1 11·5	−31 50·5	30 39·0	30 55·0	0 16·0	1 34·0	80
11	−1 3·0	−31 57·5	30 54·5	31 7·5	0 13·0	1 48·5	85
16	−0 55·0	−32 4·0	31 9·0	31 22·0	0 13·0	2 5·0	84
21	−0 47·0	−32 10·5	31 23·5	31 35·5	0 12·0	2 20·0	84
26	−0 38·0	−32 18·0	31 40·0	31 52·0	0 12·0	2 39·0	86
May 1	−0 29·5	−32 25·0	31 55·5	32 8·5	0 13·0	2 57·5	85
6	−0 20·5	−32 32·5	32 12·0	32 24·5	0 12·5	3 15·5	86
11	−0 12·5	−32 39·0	32 26·5	32 41·5	0 15·0	3 34·5	84
16	−0 3·0	−32 48·0	32 45·0	32 58·5	0 13·5	3 53·5	88
21	+0 8·0	−33 0·0	33 8·0	33 18·0	0 10·0	4 15·0	92
26	+0 19·0	−33 12·0	33 31·0	33 36·0	0 5·0	4 35·0	92
31	+0 28·5	−33 21·0	33 49·5	33 51·5	0 2·0	4 52·5	88

In the above pages and tables—

Sign + is used to signify that the error of chronometer is *fast* on G.M.T.

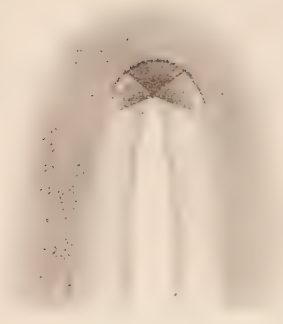
„ − „ „ „ „ *slow* „

Liverpool, 1874, *Sept.* 3.

July 1. $11^h\ 0^m$

July 3. $10^h\ 30^m$

July 4. $11^h\ 0^m$



July 6. $10^h\ 0^m$

July 7. $10^h\ 30^m$

July 8. $12^h\ 30^m$

July 13. $11^h\ 0^m$

July 14. $11^h\ 0^m$

as seen in a refractor of 10 inches aperture, 12 ft. 9 in. focal length. Power 80.

Malby & Sons, lith

July 8. 12h 30m

July 13. 11h 0m

as seen in a refractor of 10 inches aperture, 12 ft. 9 in. focal length. Power 80.

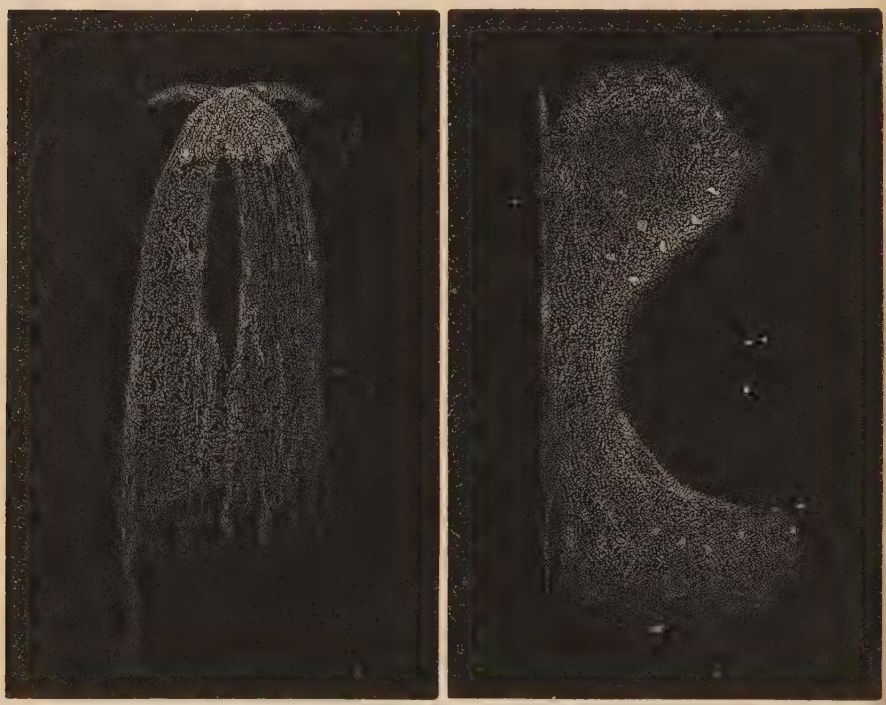
Note on Coggia's Comet. By J. M. Wilson and G. M. Seabroke.

Although it seems somewhat late to record further observations on this Comet, still we think the following Note may not be uninteresting to the Society. On the night of the 15th of July it will be recollected the Comet was seen shortly before midnight at its greatest splendour, at least it was so seen in our neighbourhood. Its tail could then be traced from near the horizon to nearly an altitude of 45°: this, expressed in real length, is not less than 20,000,000 miles. On the morning of the 22nd, at about one o'clock, we were looking over the northern horizon for any remains of the Comet that might be visible, when we clearly saw that the light near the horizon was brighter, and had a sharper outline than is usual on a summer's night. The light was not apparently concentric with the place of the Sun, but extended upwards in two not quite symmetrical branches, putting one in mind of a large bright fish's tail. The position of the boundary line of the light, as appears from our notes taken at the time, was as follows: "Its boundary line, which was rather sharply defined, stretched from *Cassiopeia* between *Perseus* and *Polaris* ($\frac{1}{3}$ of distance from former to latter) through α *Urs. Maj.* and ϵ *Urs. Maj.*, beyond which was cloud. The centre of curvature was between *Polaris* and β *Urs. Min.*; below this line there was light, and above darkness." This is shewn better by the drawing accompanying this Note. It would therefore appear that there was a light superposed on the usual glimmer of the horizon, and we can only ask, can this light have been the recurved extremity of the tail of the Comet, after the nucleus had descended far below the northern horizon? From the length of the tail on the 15th, and from the fact that it would nearly lie in a radius vector of the Earth's orbit, this seems possible.

We carefully examined the spectrum of the Comet on more than one night, and compared it with that given by the flame of a spirit lamp. The light of the flame being gradually brought into the field of the telescope, and its spectrum superposed on that of the Comet, by this method we could detect no want of coincidence in the positions of the bands. With the length of the slit in the direction of the length of the Comet, the band near "b" from the light of the leading portion of the Coma, seemed somewhat longer than the others, but whether this was due to its excess of brightness, or to an extension of the gas giving off that band in front of the Coma, was not at all certain. The bands were, as is usual with the spectrum of comets, sharply defined on the red side, and gradually shaded off towards the blue. They gradually increased in width and brightness towards the middle, where they were crossed by the continuous spectrum of the nucleus.

We submit with this note a drawing of the Comet, made by us on the night of the 14th of July at 10 to 10·15 P.M. with the $8\frac{1}{4}$-inch refractor. This may have some interest when compared

with others taken with greater apertures. It is worth adding that, to the one of us who first drew the Comet, the left-hand ear appeared distinctly brighter than the other, although to the other



who observed it after the lapse of a quarter of an hour, no such difference was visible, and this change was corroborated afterwards by the first observer. The number of visitors prevented further observations on the variation of brightness. It was also thought by us that the Comet was reddish in colour.

Note on the Brightness of μ Draconis. By the Rev. Samuel J. Johnson.

Smyth and Webb have classed this star as mag. 4. On December 4, 1872, and December 8, 1873, I was therefore surprised to find it only of the faintest class visible to the unaided eye, say mag. 5 or $5\frac{1}{2}$, of the scale Smyth uses, and so it still remains. Are we to suppose an error in Smyth's and Webb's works? Or has any observer recorded a change in the star's lustre? *

* The magnitude given in the B.A. Catalogue is 4, which is probably taken from that in Piazzi's Catalogue (Ed. 1814). Both Argelander and Heis give the magnitude 5-4, or equal to about 4·7.—[E. D.]

Corrections to the Astronomer Royal's Report on the "Preparations for the Observation of the Transit of Venus." By the Astronomer Royal.

In my report to the Society, on the "Preparations for the Observation of the Transit of *Venus*, 1874, December 8–9," I inadvertently committed some inaccuracies and omissions, of which I ask leave to insert here the corrections.

In speaking (page 7) of the delay of the Kerguelen Expedition at the Cape of Good Hope to September 18, I stated that "they ought to have left two months earlier." This was an error, incident to an oral address, as is obvious from the circumstance that one part of the observers left England on July 5. I intended to express "about a month earlier"; but I now find, on examining the Official Programme, that the delay was about a fortnight.

In pages 7 and 8, after adverting to the anxieties of the officers commanding the ships of war (in properly carrying out their instructions, as then understood) to leave Kerguelen at an early time, I omitted to state that the expedition is very deeply indebted to Commodore Sir William Hewett for his assistance at the critical time. Sir William Hewett received with kindness the Memorandum of Father Perry, and acted upon it promptly in favour of the expedition. This conduct, I believe, has been fully approved by the Admiralty. In this transaction, as in that of sending out the *Volage*, the utmost attention has been given by the Board of Admiralty and their confidential officers to the promotion of the interests of the expedition.

1874, *December* 22.

Telegrams relating to the Observations of the Transit of Venus, 1874, December 9, received by the Astronomer Royal.

Telegram, Reuter's Telegraph, Constantinople, *December* 9.

Weather rainy and dark, rendered observations impossible.

Telegram by Eastern Telegraph, from Captain Orde Browne, Mokattam, 1874, *December* 9.

The egress of *Venus* was observed at Mokattam this morning There has been much bad weather and anxiety; all well now. Contact seen through very slight haze, with Lee at about $13^h\ 22^m\ 25^s$ sidereal, and with De La Rue $13^h\ 22^m\ 21^s$. (Observe, in the Greenwich Book of Observations with the Model, my egress is always after other observers', except Mr. Gill's.) Clouds spoilt much double-image work, but many limbs and cusps were

taken; the phases closely resemble those of the model, except a line of light round the planet's edge which appeared with strong sun just after the above contacts; it perplexed me, and made me lose my best cusps; when I found that it continued two minutes, and that it would do so indefinitely, I turned to cusps.

I have exchanged bad telegraph-signals twice with Thebes, and good ones three times with Suez.

Mokattam, co-latitude 59° 58′ 14″.

Mr. Hunter, from Suez, says, "Sky cleared partly a few minutes before contact; contact satisfactorily observed, and a considerable number of micrometer measurements made."

Captain Abney says, from Thebes, "Beautiful morning, Sun rather shaky at first, nice and sharp at time of contact, and good observations, though differing slightly in time; sun pictures good; the fifty photographs in Jannsen's slides include internal contact; external contact not taken; no black drop apparent in photographs after careful examination."

Telegram by Reuter's Telegraph, Alexandria,
Wednesday, 1874, *December* 9.

British Government Expedition for observing the Transit of *Venus* in Egypt in conjunction with the Associated Astronomers has been completely successful. The weather has been bad and the sky clouded for the last two days, and this morning the view was interrupted by misty clouds which, however, opened sufficiently to exhibit *Venus*, but not to take measurement until critical instant of contact.

The following made successful observations:—The Chief Greenwich Station at Mount Mokattam, viz., Captain Browne and Mr. Newton; at Suez, Mr. Hunter; at Thebes, Colonel Campbell and Messrs. Auwers and Döllen. A series of fifty photographs were taken at successive instants, including the moment of contact, by Captain Abney. Sir G. B. Airy had exchanged time-signals by telegraph with Captain Browne at Mokattam; Messrs. Ellis and Hunter observing the ends of the submarine cable.

Note by the Astronomer Royal. Longitude of Mokattam station, $2^h\ 5^m\ 6^s{\cdot}44$, very approximately.

Telegram from John Burns, Glasgow,
Wednesday, 1874, *December* 9, 2 30 *p.m.*

The following telegram regarding the transit of *Venus* has been this moment received: "Barker, Alexandria, to John Burns, Castle Wemyss, Wemyss Bay, Scotland. Egress commenced eight o'clock five minutes forty seconds one-tenth; final egress between eight o'clock thirty-three minutes and eight o'clock thirty-four minutes forty-six seconds; clouds covering the Sun prevented ascertaining precise exit. The Planet looked like a black ball with red under lower circumference. Observations of Mr. Perona taken north, thirty-one degrees, Sun now shining."

Telegram by Reuter's Telegraph, Bushire.

Transit at Indore beautifully observed; interval from commencement to end. Apparent contact, $4^h\ 37^m\ 32^s$; interval between two internal contacts, $3^h\ 42^m\ 56^s$. No black drop appeared.

Telegram from Colonel Tennant, Roorkee, by Teheran line.

Observations here successful; one hundred photographs taken.

Telegram by Reuter's Telegraph.

Observation, at Calcutta, excellent; mean time of ingress of centre was seven fifty-six; middle, ten five; egress of centre, twelve thirteen. At Madras satisfactory observations were almost impossible; endeavours frustrated by cloudy weather. At Kurrachee first external contact happened before sunrise at $10^m\ 26^s$ past 6; first internal contact was when Sun was about four or five of his diameters above horizon at 13^m before 7; second internal contact about ten thirty-five; last internal contact about eleven three, when Planet quitted Sun not far from its highest point.

Telegram by Reuter's Telegraph, Shanghai, *Wednesday*, 1874, *December* 9.

During the period of the transit of *Venus* the weather here was overcast and the Sun obscured.

Telegram by Reuter's Telegraph, Nagasaki, *Wednesday*, 1874, *December* 9.

Weather this morning was magnificent, and the transit observations in Japan were most successful: Observed contacts were obtained by revolver photograph [Janssen's]; fine images in the telescope, no ligament, *Venus* seen over Sun's corona; glass photographs and silver plates cloudy at intervals; two members observed successfully Kobe [Hiogo].

Telegram by Reuter's Telegraph, Berlin, *December* 10.

A telegraphic report from Ispahan of the German Expedition sent out to observe the transit of *Venus* has been received. It states that, although the weather was unfavourable, fourteen serviceable photographs were secured; the observations of the contact were materially hindered by the clouds. From St. Petersburg, 10th December, the conditions were favourable at Wladiwostock, Yokohama, Arrianda; partially favourable at Possiet, Habarowka, Tschita; totally unfavourable at Omsk, Blagoweschtschensk, Orenburg, Kazan, Uralsk, Astrachan, Kertch, Tiflis, Eriwan, and Naktritchevan. Intelligence is still wanting from twelve stations.

Telegram from Struve, Pulkowa, to Sir George Airy.

Transit of *Venus*: Full success, Wladiwostock, Yokohama, Tschita, Orianda, near Jalta; partial success, Possiet, Habarowka; completely cloudy, Blagoweschtschensk, Omsk, Orenburg, Uralsk, Kazan, Astrachan, Kertch, Tiflis, Eriwan, Naktritchevan. From fifteen stations reports are outstanding.

Telegram from Foerster, Berlin.

Station Ispahan, has got nineteen good photographs in very unfavourable weather.

Telegram by Reuter's Telegraph, Hiogo, 1874, *December* 9.

Transit of *Venus* successfully observed here: Adelaide, 9th of December, the observations made here have been partially successful; Hobart's Town, December 9th, the American astronomical party here have had partial success in their observations; Melbourne, December 9th, Professor Ellery reports from Melbourne Observatory, that all phases of the transit of *Venus* have been successfully observed.

Telegram by Reuter's Telegraph.

Mr. Ellery, at Melbourne Observatory, reports that all phases of the transit of *Venus* have been successfully observed.

Telegram by Reuter's Telegraph, Constantinople, *December* 11.

At Beyrout, the observations of the transit of *Venus* were very successful.

Telegram from Mr. Struve, Pulkowa, *December* 13.

The transit successfully observed at Nertschinsk, Teheran, partially at Kiaktcha; photographs at Possiet after development prove satisfactory.

Telegram from Mr. Russell, Sydney, *December* 10.

Transit well observed, ingress and egress; 390 (?) photographs; 16 Janssen's plates; no black drop.

Telegram from Major Palmer, Christchurch, New Zealand, *viâ* Sydney, *December* 17.

English, nothing valuable anywhere, clouds; Americans got ingress and photographs till near third contact; nobody egress.

Telegram from Capt. Tupman by Anglo-American Telegraph.

Transit of *Venus* well observed, clear weather at Honolulu and Waimea, Atooi; cloudy at Kailua, Owhyhee; sixty photo-

graphs; Janssen failed; internal contact uncertain several seconds; complete disk of *Venus* seen twelve minutes before; 120 micrometer measures.

Telegram by Reuter's Telegraph.

The Netherlands Expedition to observe the transit of *Venus* at Réunion, only half successful on account of obscure sky.

Computations in regard to the Transit of Venus.

By J. R. Hind, Esq.

(*Extract from a Letter to the President.*)

The only observed time of either internal contact of *Venus* pretending to any accuracy which we have yet seen here, is that published in the *Times* of (December 10) as having been observed at Alexandria.

I have had the curiosity to compare this observation with the result of accurate computations; first, with Le Verrier's elements of Sun and Planet, and the semi-diameters recommended by him at p. 40 of the *Introduction to Tables of Venus*, and secondly with Carlini's Sun and Lindenau's *Venus*, and the old *Nautical Almanac* values for semi-diameters.

The observed time of second internal contact at Alexandria to which I have referred is

Dec. 8 at $20^h\ 5^m\ 40^s{\cdot}1$ Local Mean Time.

With Le Verrier's elements, I find it

Dec. 8 at $20^h\ 5^m\ 39^s$

Correction to computed time = $+1^s$

(so very small a difference is no doubt to a certain extent fortuitous.)

With Carlini's and Lindenau's elements, I find it

Dec. 8 at $19^h\ 50^m\ 46^s$

Correction to computed time = $+14^m\ 54^s$.

This comparison shows to how great extent Le Verrier's theories of Sun and Planet have benefited us in respect to this year's transit of *Venus*.

As you may probably be in possession to-day of observed times at one of the normal Egyptian stations, I add my results for Cairo and Thebes, deduced, with every care, from Le Verrier's tables and semi-diameters. The original calculations for Alexandria also accompany this for your satisfaction.

I recently made a close examination of the Transits of 1761 and 1769, after determining elements from Le Verrier's Tables. As was to be expected, I arrived at very similar conclusions to those he has obtained in his work for the *Tables of Venus*.

With regard to semi-diameters, I now think it will probably be found that we should have made closer predictions of the times of contact by adopting the values to which I have alluded. As Le Verrier remarked to me, "the two must go together," and up to the last transit we had no means of ensuring this, except by adopting the diameters deduced from the transits of last century.

With Le Verrier's values for semi-diameters (*Introduction to Tables of Venus*, p. 40) :—

Cairo.	2nd Internal Contact	Dec. 8	$20^h\ 11^m\ 8^s$	Local M.T.
	$2^h\ 5^m\ 1^s{\cdot}4$ E.		344°·7	N. to E.
	$+30°\ 2'\ 4''$		32°·3	Vert. to E.
Thebes. (Ruins of Luxor)	2nd Internal Contact	Dec. 8	$20^h\ 16^m\ 0^s$	Local M.T.
	$2^h\ 10^m\ 21^s{\cdot}1$ E.		344°·8	N. to E.
	$+25°\ 41'\ 57''$		35°·3	Vert. to E.
Alexandria.	2nd Internal Contact	Dec. 8	$20^h\ 5^m\ 39^s$	Local M.T.
	$1^h\ 59^m\ 25^s{\cdot}65$ E.		344°·7	N. to E.
	$+31°\ 11'\ 47''$		32°·1	Vert. to E.

3 *Verulam Buildings, Gray's Inn, W.C.*, 1874, *Dec.* 11.

Extract from a Letter from R. J. Ellery, Esq., F.R.S. *to* Rev. Dr. T. R. Robinson, F.R.S.

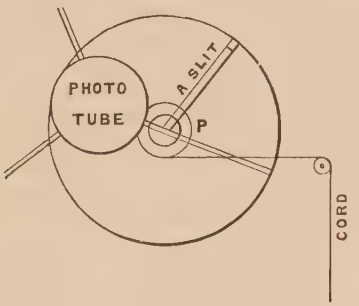
We are now in the midst of a systematic series of observations of Sir John Herschel's figured Nebulæ, many of which appear to have greatly changed since the Cape drawings, not only in shape and character, but also apparently in position with respect to some of the stars Sir John figured with the Nebulæ. As soon as I have got over the present pressure, I will send you a list of the most remarkable differences that have been observed. . . . We have still got Coggia's Comet in view ; and Mr. Turner made a sketch from it last night with the great Melbourne telescope —it is too faint now for smaller instruments.

For the transit of Venus I have decided to employ the great Melbourne telescope for photography at the primary focus. I proposed to have a disk (with radial slits) 19″ diameter rotating

upon an arm two inches outside the mouth of plate-holder tube—so that the radial slit will traverse just in front of the photo-apparatus in same place as the Moon shutter now is. The rotation of the disk will be done by a cord and weight wound half-a-turn around a small pulley on the axis of the disk, the cord being led down to the eye-end by small guide pulleys.

In setting the shutter, the disk is turned till the slit is in position; shown when the cord is wound half-a-turn on the pulley P and the disk held by a detent; on detaching the detent, the disk rotates, carrying the slit across the photo-apparatus, where it is caught by the detent falling into another notch.

The cord passes its dead point and a little beyond it, by the momentum of the disk, and winds up the weight slightly the contrary way; and it is thought that a moderately uniform motion of the slit across the plate-holder may be got in this way. I hope to be able to report our success with this arrangement to you by the next mail.



Melbourne,
1874, *October* 7.

On Personality in observing Transits of the First and Second Limbs of the Sun. By E. Dunkin, Esq.

In the *Monthly Notices*, vol. xxix. pp. 259–268, a short paper of mine is inserted, the object of which was to draw the attention of astronomers to a well-marked personality exhibited in the observations of the first and second limbs of the Moon, and to the fact that this personality in the case of some of the observers amounts to a very important quantity. These personal differences are detected in the tabular errors of R.A. of the Moon, determined both from the meridional and extra-meridional observations, and the steadiness of the results from year to year is a decided proof of the reality of the personality.

This question has again come before the Society, bearing upon

the determination of the longitudes of the stations at which the transit of *Venus* has been observed, and a statement was made at the meeting held on November 13, on the authority of Prof. Asaph Hall, that in the determination of the longitudes from observations of the Moon and Moon-culminating stars, the resulting longitude may be affected by a residual error equal in some cases to four seconds of time, owing to the personal discordances introduced by the different methods of estimation employed by the opposite observers during the transit of the Moon's limbs. This opinion was formed by Prof. Hall from a comparison of the final results of certain well-known determinations of longitude made both by the telegraphic and lunar methods.

Although it seems scarcely possible that so large an error can be outstanding in the resulting longitude by the lunar method, when it depends on a large and *equal* number of observations of the first and second limbs of the Moon, yet it is shown in my comparison of the mean errors of tabular R.A. deduced from the observations on the meridian and with the Altazimuth, that the amount of personality in some observers, different in both limbs, is sufficiently great to account for some of the discordance which has been found between the telegraphic and lunar methods. In the determination of the longitudes of the stations where the transit of *Venus* has been observed, especially of those longitudes which must depend principally upon lunar observations, it is hoped, for this reason, that means will be found to determine the value of the personality in observing the Moon's limbs for each of the observers employed in making the observations from which the longitudes are to be deduced.

In relation to this subject, and to show whether the personality detected in observing the limbs of the Moon is confined to that object, the Astronomer Royal suggested to me that it would be interesting, and at the same time important, to investigate the amount of personal differences between the meridional observations of the first and second limbs of the Sun, as observed at Greenwich by the principal observing assistants, treating the subject in a similar manner to that adopted by me in my former investigation. I had some time ago thought of doing this, but on a casual examination of the errors of the tabular R.A. of the centre of the Sun, and also of the tabular duration of passage, the general smallness of the errors of the tables, positive and negative, made me hastily conclude that there could be no well-marked personality detected out of such small numbers. But I now find that I was labouring under a wrong impression of what could be gathered from a systematic examination.

The numbers, which form the basis of the present investigation may be found in the first pages of the *Planetary Results*, one of the sections in the volume of *Greenwich Observations*. In the reductions it is not usual to compare the observed R.A. of the two limbs of the Sun separately with the tabular R.A. taken from the *Nautical Almanac*, but to take the mean of the two transits, and then to this mean, which represents the clock-time of

passage of the centre of the Sun, to apply the corrections for the instrumental and clock-errors. To determine the error of tabular R.A. of each limb, the numbers are, however, ready to hand in columns, so that by merely subtracting first the error of observed semi-duration of passage from the tabular error of R.A. of the centre, for the tabular error of R.A. of the first limb, and, secondly, by adding these two quantities together for the tabular error of R.A. of the second limb, we have all that is required. These results are therefore the same as if we had reduced the transit of each limb separately to the end. The numbers being extracted from the printed columns, and separated according to the observer, for each year, and the means of the resulting errors for the two limbs taken, the numbers which are given in Tables I and II are obtained. I may here state that I have confined my inquiry to the ten years included between 1864 and 1873, the former year being that in which M. Le Verrier's Solar Tables were first used in the *Nautical Almanac*.

Table I.

Mean Tabular Error in R.A. of Sun from Observations of the First Limb.

Year.	Mr. Dunkin. (D.) s	Mr. Ellis. (E.) s	Mr. Criswick. (C.) s	Mr. Lynn. (L.) s	Mr. J. Carpenter. (J. C.) s	Mr. H. Carpenter. (H. C.) s
1864	−0·002	−0·046	+0·006	...	−0·156	...
1865	− ·036	− ·033	+ ·062	...	− ·110	...
1866	− ·037	− ·136	+ ·007	...	− ·135	+0·001
1867	− ·063	− ·161	+ ·028	...	− ·154	...
1868	− ·024	− ·108	+ ·005	...	− ·111	− ·020
1869	− ·018	− ·063	+ ·021	...	− ·125	+ ·042
1870	...	− ·080	+ ·008	−0·030	− ·137	− ·010
1871	...	− ·065	− ·008	− ·035	− ·156	− ·044
1872	...	− ·138	− ·024	+ ·033	− ·165	− ·098
1873	...	− ·081	+ ·015	+ ·016	...	...

Table II.

Mean Tabular Error in R.A. of Sun from Observations of the Second Limb.

Year.	D. s	E. s	C. s	L. s	J. C. s	H. C. s
1864	+0·041	+0·010	+0·014	...	+0·020	...
1865	+ ·016	+ 062	+ ·057	...	+ ·076	...
1866	− ·003	− ·011	− ·021	...	+ ·008	+0·161
1867	+ ·014	− ·037	+ ·086	...	+ ·019	...
1868	+ ·040	− ·017	+ ·035	...	+ ·115	+ ·174
1869	+ ·083	+ ·038	+ ·033	...	+ ·068	+ ·162
1870	...	+ ·016	+ ·053	+0·077	+ ·056	+ ·194
1871	...	+ ·041	+ ·038	+ ·027	− ·013	+ ·186
1872	...	+ ·023	+ ·021	+ ·083	− ·026	+ ·086
1873	...	+ ·024	+ ·020	+ ·096	...	...

The next table exhibits the mean tabular error of each observer's group both for the first and second limbs, and also the mean error of the tabular duration of passage of the diameter of the Sun. I also insert the mean tabular error of the centre of the Sun, as deduced from the mean of the separate groups for 1st L. and 2nd L., by using each observer's own value of the mean duration of passage:—

Table III.

	Mean Tabular Error in R.A. of Sun.			*Error of Tabular Duration of Passage of Diameter.*
	1st L.	2nd L.	Centre.	
	s	s	s	s
D.	−0·030	+0·032	+0·001	+0·062
E.	− ·091	+ ·015	− ·038	+ ·106
C.	+ ·012	+ ·034	+ ·023	+ ·022
L.	− ·004	+ ·071	+ ·034	+ ·075
J. C.	− ·139	+ ·036	− ·051	+ ·175
H. C.	− ·022	+ ·160	+ ·069	+ ·182

In the above table, the differences to be noticed, especially in the errors for the first limb, are too great to be accidental; on the other hand, there can be no doubt of the reality of a well-marked personality in Mr. J. Carpenter's estimation of the first limb, and in Mr. H. Carpenter's estimation of the second limb, while the differences of the numbers generally represent, I believe, the relative differences or habits of all the observers. To show that there is the same method employed in observing the limbs of the Sun and Moon, although the glare produced by the Moon in the field of the telescope is greater than that produced by the Sun, whose light is sufficiently moderated by being viewed through coloured glasses, I place here the differences between the errors of the tabular R.A. of the Sun determined by Mr. Criswick and those of D, E, and J. C, side by side with the corresponding differences of the tabular errors of the R.A. of the Moon extracted from my former paper (vol. xxix. p. 267):—

Table IV.

	Tabular Errors of R.A. of Sun.		*Tabular Errors of R.A. of Moon.*			
	Transit-Circle Obs.		Transit-Circle Obs.		Altaz. Obs.	
	1st L.	2nd L.	1st L.	2nd L.	1st L.	2nd L.
	s	s	s	s	s	s
C.−D.	+0·052	+0·002	+0·034	+0·032	−0·047	−0·028
C.−E.	+ ·103	+ ·019	+ ·112	+ ·077	+ ·104	+ ·048
C.−J.C.	+ ·150	− ·001	+ ·132	+ ·038	+ ·207	+ ·004

This comparison gives good evidence that the different habits of observing the limbs of the Sun and Moon by these four observers are essentially the same in the observations of both objects. In

Tables V and VI which follow, the numbers represent the comparison of the tabular errors of the first and second limbs of the Sun from each observer's annual mean in Tables I and II, referred to the corresponding tabular errors from the annual mean of Mr. Criswick's observations. The steadiness of the personality is very clearly shown.

TABLE V.

Comparison of the Tabular Error of R.A. of Sun determined by each Observer, referred to Mr. Criswick as the Standard, from Observations of the First Limb.

Year.	C.–D. s	C.–E. s	C.–L. s	C.–J.C. s	C.–H.C. s
1864	+0·008	+0·052	...	+0·162	...
1865	+ ·098	+ ·095	...	+ ·172	...
1866	+ ·044	+ ·143	....	+ ·142	+0·006
1867	+ ·091	+ ·189	...	+ ·182	...
1868	+ ·029	+ ·113	...	+ ·116	+ ·025
1869	+ ·039	+ ·084	...	+ ·146	− ·021
1870	...	+ ·088	+0·038	+ ·145	+ ·018
1871	...	+ ·057	+ ·027	+ ·148	+ ·036
1872	...	+ ·114	− ·057	+ ·141	+ ·074
1873	...	+ ·096	− ·001	...	...
Means	+0·052	+0·103	+0·002	+0·150	+0·023

TABLE VI.

Comparison of the Tabular Error of R.A. of Sun, determined by each Observer, referred to Mr. Criswick as the Standard, from Observations of the Second Limb.

Year.	C.–D. s	C.–E. s	C.–L. s	C.–J. C. s	C.–H. C. s
1864	−0·027	+0·004	...	−0·006	...
1865	+ ·041	− ·005	...	− ·019	...
1866	− ·018	− ·010	...	− ·029	−0·182
1867	+ ·072	+ ·123	...	+ ·067	...
1868	− ·005	+ ·052	...	− ·080	− ·139
1869	− ·050	− ·005	...	− ·035	− ·129
1870	...	+ ·037	−0·024	− ·003	− ·141
1871	...	− ·003	+ ·011	+ ·051	− ·148
1872	...	− ·002	− ·062	+ ·047	− ·065
1873	...	− ·004	− ·076	...	...
Means	+0·002	+0·019	−0·038	−0·001	−0·134

These tables, perhaps more than in any of the preceding, exhibit clearly to the eye the remarkable differences between C and J. C in Table V, and between C and H. C in Table VI. There is, therefore, this curious fact, that whereas Mr. J. Carpenter's personality lies almost wholly in observations of the first limb, Mr. H. Carpenter's personality lies almost wholly in observations of the second limb. The effect of these peculiar personal habits of estimation of the solar limbs is that the position of the centre of the Sun, according to the two observers, differs $0^{s}.12$, or nearly two seconds of arc.

As the numbers in the preceding tables show that the errors of the R.A. of the Sun and Moon, deduced from observations made with the Transit-circle by D, E, C, and J. C, are larger for the first limb than for the second limb, and that there is a general resemblance in the magnitude of the differences between the results of the separate observers for both objects, it may be assumed that the personalities detected by the present and former comparisons are as real as those which have been so long known in connection with transits of stars. Having a knowledge of this fact, which, I believe is the chief cause of the discrepancy found between the resulting longitudes determined by the telegraphic and lunar methods, all future standard determinations of longitude by the lunar method should be corrected for any personality in observing the Moon's limb, such as I have exhibited in this paper as existing in the case of observers who have had such great experience in this class of observation.

In preparing the tables which are given above, I included originally the observations made by several occasional observers. They were, however, comparatively so few in number, that I resolved to confine myself to those of the principal assistants, with the addition of Mr. H. Carpenter, whose observations have extended over several years. So far, however, as I could judge from the limited number of observations of the junior observers, the same decided personalities are visible, and in amount fully equal to any contained in this present paper. I have also not included Mr. Downing's one year's observations, in 1873, in the tables, but they show that his personality exists in transits of the second limb, the respective tabular errors of R.A. being $-0^{s}.027$ and $+0^{s}.143$.

I am not able to point out, or even to suggest, any cause for these peculiar habits of observing the limbs of the Sun and Moon, except that they may possibly arise from the different personal methods of estimating by the eye the positions of the two limbs in transit, as affected by irradiation. Most observers, however, have an impression that transits of the first limb are not so easily made as those of the second limb; owing in some measure to the first effect of the glare on the eye, which causes the transit-wires to appear fainter in the former observations than when the second limb is approaching, when the wires are seen projected as black lines on a luminous background. That

the personality has its origin in the eye is most probable, as in all the observations used in this investigation the transits were recorded automatically on the chronograph.*

Kenwyn, Kidbrooke Park Road,
Blackheath, 1874, Dec. 10.

* Dr. Robinson has remarked, in the Introduction to the *Armagh Catalogue*, on the existence of a variable personal equation in observing transits of the first and second limbs of the Moon, and he has given examples of the effect of this personality, by comparing the difference of longitude, determined by the method of Moon-culminating stars, between Armagh and the Observatories of Greenwich, Dublin, Cambridge, Königsberg, and Paris, as found from observations of the first and second limbs of the Moon. His discordances between the separate results are great, but the number of observations is generally far too small to obtain a trustworthy determination. Dr. Robinson concludes that these personal differences are owing solely to irradiation, the origin of which is in the eye. He has, from experiment, found that the irradiation of the limb is diminished considerably by strongly illuminating the field of view; and, by reducing the aperture of the object-glass by a semi-transparent diaphragm of oiled paper, which should fill the field with scattered light, he has very largely reduced the error in the Sun's observed diameter produced by irradiation.

Phenomena of Jupiter's Satellites, observed at Mr. E. Crossley's Observatory, Bermerside, Halifax, with a 9½-inch Equatoreal Refractor by Cooke.

By Mr. Joseph Gledhill, F.R.A.S., F.G.S. etc.

Date. 1873.	Satellite.	Phenomenon.	G.M.T. h m s	Remarks.
May 6	I.	Sh. I. int. contact	8 32 20	Considerable motion; not a good obs.
		Tr. E. int. contact	9 31 3·5	Bad definition; not good observations.
		bisection	9 33 2·3	
		ext. contact	9 34 30	
May 9	II.	Tr. I. ext. contact	8 57 0·5	The first and last are the best.
		bisection	8 59 30·4	
		int. contact	9 1 0·8	
May 11	II.	Ec. R. first seen	9 29 6·2	Good.
		fully bright	9 31 0	Pretty good.
May 12	I.	Occ. D.	11 49 0	Quite gone; cloud prevented a good view.
May 13	I.	Tr. I. ext. contact	9 9 0	Good.
		bisection	9 11 3·5	Pretty fair.
		int. contact	9 12 6·3	Good.
	I.	Sh. I. int. contact	10 28 4	Pretty fair observation.
	I.	Tr. E. ext. contact	11 30 30	Planet low; definition bad.
May 14	I.	Ec. R. first seen	9 51 15·5	Good.
		fully bright	9 53 5	Fair.

Date.	Satellite.	Phenomenon.	G.M.T. h m s	Remarks.
1873.				
May 21	I.	Occ. D. first contact	8 11 30·4	Fair.
		bisection	8 13 30	Pretty good.
		disappeared	8 15 0	Good.
	III.	Sh. E. just off	8 50 0	Definition not good; obs. difficult.
May 22	I.	Tr. E. int. contact	7 50 0	Not Good.
		ext. contact	7 53 30	Good.
	I.	Sh. E. last seen	9 8 0	Bad definition; uncertain obs.
May 24	IV.	Ec. R. first seen	10 47 3·2	Good.
		fully bright	10 49 0	Uncertain.
June 6	I.	Ec. R. first seen	10 5 35	Good.
		fully bright	10 7 35	Uncertain.
1874.				
Feb. 19	II.	Ec. D. began to fade	13 42 0	Pretty good.
		disappeared	13 44 46	Good.
Mar. 7	III.	Ec. D. fading away	10 22 0	Uncertain.
		disappeared	10 25 1	Good.
Mar. 9	I.	Ec. D. fading	9 52 0	Uncertain; very bad night.
		fading fast	9 53 0	
		disappeared	9 54 5	Good.
	II.	Occ. R. first seen	11 12 30	Pretty good.
		ext. contact	11 40 30	Fair; bad definition.
	I.	Occ. R. first seen	12 16 15	Observations not good; boiling.
		ext. contact	12 19 30	
Mar. 10	I.	Sh. E. first contact	9 17 10·3	Definition very bad.
		bisection	9 18 2·7	
		last contact	9 20 5	
	I.	Tr. E. first contact	9 25 5·3	The last observation is the best; the definition is very bad indeed.
		bisection	9 26 2·7	
		last contact	9 30 3	
Mar. 18	II.	Tr. E. int. contact	7 59 0	Violent boiling; obs. not good.
		bisection	8 2 7·5	Not good.
		ext. contact	8 5 3·5	Pretty fair.
	I.	Occ. R. last contact	8 28 30	Fair.
Mar. 24	I.	Tr. I. first contact	10 41 1·3	Not so good.
		bisection	10 43 0	Not so good.
		last contact	10 45 5·5	Pretty good.
	I.	Sh. I. bisection	10 55 0	Difficult, owing to motion.
		int. contact	10 55 30	
	I.	Tr. E. first contact	12 55 3·5	Pretty good.
		bisection	12 57 0	Uncertain.
		last contact	12 58 2·5	Fair.

Date. 1874.	Satellite.	Phenomenon.	G.M.T. h m s	Remarks.
	I.	Sh. E. first contact	13 7 3·2	Fair.
		last contact	13 9 5·4	Fair.
Mar. 25	III.	Tr. I. first contact	7 18 0	Fair.
		bisection	7 21 3	Pretty good.
		int. contact	7 25 5	Pretty good.
	II.	Tr. I. first contact	7 41 40	Very fair.
		bisection	7 43 30	Very fair.
		int. contact	7 45 0	Very fair.
	I.	Occ. D. first contact	7 54 0	Much motion; fair observations. Observed by Mr. Crossley.
		bisection	7 55 0	
		disappeared	7 58 0	
	II.	Sh. I. bisection	8 4 0 Fair	Observed by Mr. Crossley.
		int. contact	8 5 30 Fair	
	II.	Sh. E. first contact	10 37 10	Much boiling just now.
		bisection	10 39 45	
		last contact	10 42 0	
	III.	Sh. E. first contact	11 7 0 Pretty good	Observed by Mr. Crossley.
		bisection	11 11 30 Pretty good	
		last contact	11 14 0 Pretty good	
	II.	Tr. E. first contact	10 10 30	Uncertain.
		bisection	10 15 0	Not good.
		last contact	10 17 5	Fair.
	I.	Ec. R. first seen	10 20 40	Good.
		fully bright	10 22 30	Fair.
	III.	Tr. E. first contact	10 21 0	Bad, perhaps.
		last contact	10 24 30	Pretty good.
Mar. 31	I.	Tr. I. first contact	12 24 0	Good.
		bisection	12 27 0	Not so good.
		last contact	12 28 30	Good.
April 3	II.	Ec. R. first seen	7 57 51	Very good.
		still dim	7 59 36	Good.
		fully bright	8 1 36	Good, perhaps.
April 8	I.	Occ. D. first contact	11 22 40	Very fair.
		bisection	11 24 30	Good, perhaps.
		disappeared	11 26 40	Very good.
April 10	I.	Ec. R. first seen	8 37 40	Probably not very good; cloudy.
April 17	II.	Occ. D. first contact	9 7 0	Probably too early.
		bisection	9 9 5	Fair.
		disappeared	9 11 2·5	Pretty good.

Date. 1874.	Satellite.	Phenomenon.	G.M.T. h m s	Remarks.
	I.	Ec. R. first seen	10 31 0	Good.
		fully bright	10 34 0	Uncertain.
April 23	I.	Tr. I. first contact	12 10 0	Pretty good.
		bisection	12 12 0	Pretty good.
		last contact	12 13 30	Perhaps not good.
May 1	I.	Oc. D. first contact	11 9 30	Fair.
		bisection	11 11 0	Not reliable.
		disappeared	11 12 30	Good.
May 2	I.	Tr. I. first contact	8 26 3	Pretty good.
		bisection	8 28 5	Not reliable.
		int. contact	8 30 2	Very fair; cloudy.
	I.	Sh. I. bisection	9 25 0	Not good; too much cloud.
			9 27 0	Not good.
	I.	Tr. E. inner contact	10 41 0	Cloudy; uncertain.
		bisection	10 43 2	Cloudy; uncertain.
		ext. contact	10 44 5	Pretty good.
May 8	I.	Occ. D. first contact	12 58 0	Planet very low; bad definition.
		disappeared	13 0 0	Observation not good.
May 10	I.	Ec. R. first seen	10 44 30	An uncertain observation; cloudy.
May 17	I.	Occ. D. first contact	9 15 1·5	Good.
		bisection	9 17 0	Uncertain.
		disappeared	9 18 30	Very good.
June 8	III.	Occ. D. first contact	8 49 0	Fairly good.
		bisection	8 54 0	Uncertain.
		disappeared	8 57 5	Very good.
June 10	I.	Tr. E. inner contact	8 54 0	Not good.
		bisection	8 56 0	Not good.
		last contact	8 58 30	Fairly good; possibly early.

On a New Astrometer. By E. B. Knobel, Esq.

Having been for some time engaged in a work which necessitates the determination of the relative magnitudes of Telescopic Stars, I have been led to devise the following contrivance for doing so.

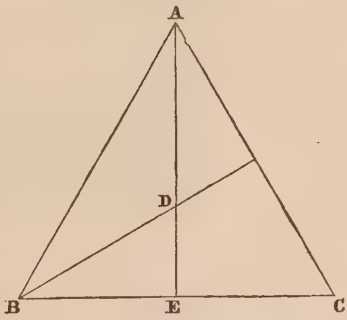
It is a well-known fact that an equatoreal triangular aperture gives very good definition. Sir John Herschel says,* "The triangular aperture," or diaphragm, which admits the light

* *Cape Observations*, p. xvi.

through an opening concentric with the speculum in the form of an Equilateral Triangle, to whose use as a means of separating close double stars continual reference will be found in the following pages, affords an elegant example of this theory,* in the sharpness of the central disk which it produces, and the absence of all appendages other than six perfectly straight delicate rays running off at angles of 60° from the disk. In a letter addressed, December 24, 1834, to the late Captain B. Hall, of which I retain a copy, I find an observation of *Canopus*, with such an aperture, and a magnifying power of 1,200, thus described :—"The disk is an *exact circle*, and the six rays which such an aperture always gives, are perfectly straight, delicate, brilliant lines, like brightly illuminated threads running far out beyond the field of view, and (what is singular) capable of being followed, like real appendages to the star, long after the star itself had left the field. In examining stars to see if they are close double, I always apply the triangular aperture. It reduces the disk to hardly more than a third of their size, and gives them a clearness and perfection incredible without trial." †

My contrivance is simply an equilateral triangle aperture, which can be increased or diminished within the limits of the triangle inscribed in the telescope tube and zero.

It is based upon the simple mathematical principle that sine $30° = \frac{1}{2}$. If ABC be an equilateral triangle, and the centre D of the triangle be found by bisecting two of the angles; then the perpendicular DE is to the hypothenuse DB as 1 to 2; and as DB=AD, therefore DE $= \frac{1}{2}$ AD.



I have therefore constructed an equilateral triangle of two plates: one containing the angle BAC, and the other forming the base of the triangle. These two plates are connected by a screw-shaft, the peculiarity of which is, that the lower portion, which carries the socket of the Base-plate, is a *left-handed* screw, and the upper part of the shaft, carrying the Angle-plate, is a *right-handed* screw. Moreover, the pitch of the lower screw, which I may call the Base-screw, is exactly $\frac{1}{2}$ that of the upper or Angle-screw.

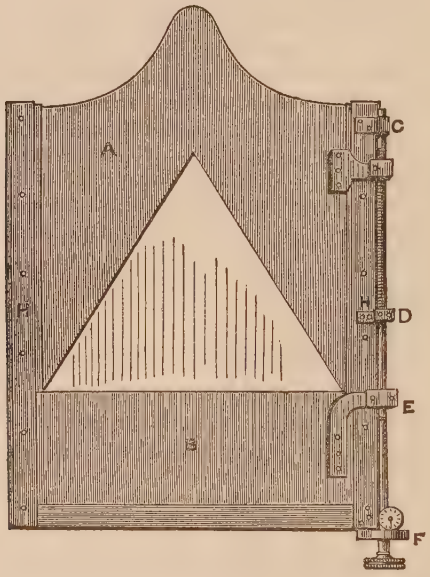
It will be seen, therefore, that, in consequence of the screws being opposite in kind, the rotation of the shaft in one direction

* The optical Law of Interferences.

† "Limiting the aperture of the object-glass or speculum to its inscribed equilateral triangle is often useful in stellar observation, as it reduces the 'spurious disks' of stars to very small points."—Herschel on "The Telescope," *Ency. Brit.*

will cause the plates to either approach or recede from each other, whilst the pitch of the screws being as 1 to 2, the rate of motion of the plates will be in that proportion. By these means the aperture is kept always equilateral and concentric with the mirror.

The drawing needs little description. A is the Angle-plate, which slides under B the Base-plate, both working between guides HH on each side. CF is the screw-shaft, of which the part CD is the right-handed screw $\frac{1}{20}$ inch pitch, and DE is a left-handed screw $\frac{1}{40}$ inch pitch; F is an ordinary micrometer head.



The instrument has been made for me by Mr. Browning, and it thoroughly fulfils all the required conditions. The motion is quite smooth and easy; the concentricity of the triangle is practically perfect, and I consider it a piece of fine mechanical work, which does Mr. Browning the greatest credit.

To use the instrument, it is necessary to be able to determine the area of the aperture available for light. I have therefore had the base of the triangle graduated each way from the centre, which will at once give by inspection half the side of the triangle. The micrometer head, fitted on the shaft, will also give the side of the triangle still more accurately, whence the area will be obtained by the formula $\frac{\sqrt{3} \times \text{side}^2}{4}$.

My telescope being a Newtonian Reflector, this area *minus* the area of the circle of the plane gives the area available for

light. The aperture can thus be easily calculated down to the point where the sides of the triangle form tangents to the circle of the plane. But when the triangle is reduced so that the sides cut off segments of this circle, the calculation is rather more complicated. And here I have been much indebted to Mr. Gray for furnishing me with a simple formula for the calculation of the available aperture under these circumstances, which I beg to subjoin to this paper. Practically, however, the instrument will measure the versed size of the segment in terms of micrometer parts, and the radius of the circle being known, the area of the segment can be found by a table of Areas of Segments, such as that given in the *Encyclopædia Britannica* (Art. 'Mensuration').

The aperture can then be calculated from the formula:

Aperture = area of triangle − area of circle of plane + 3 area of segment.

The instrument fits readily on to the end of the telescope tube, and is conveniently accessible to the hand of the observer, and it is easy for him to gradually diminish the aperture to the extinction of a star's light, without moving his eye from the eye-piece.

In the last century Bailly proposed to determine the brightness of each star upon strict photometrical principles, and suggested that all stars should be observed with the same telescope, and that in every instance the aperture should be diminished until the star just ceased to be visible. Professor Grant, in his *History of Physical Astronomy*, says, that "this mode of determining the relative brightness of the stars is unexceptionable in point of theory, but practical difficulties have hitherto stood in the way of its adoption."

My Astrometer endeavours to comply with some of these conditions; and I therefore venture to express a hope that to those observers who take up this valuable but neglected branch of astronomy, and to those who study variable stars, it may be found not altogether useless.

The Heath, Stapenhill, Burton-on-Trent,
1874, *December* 10.

An investigation of the area of the effective aperture was furnished by Mr. P. Gray. We have an equilateral triangle, the side of which is $= s$, and a concentric circle radius $= r$, wholly interior to the triangle, or else cutting its three sides; in the former case, the aperture in question is

= area of triangle − area of circle;

in the latter case, it is

= area of triangle − area of portion within the triangle,

viz. this is

= area of triangle − area of circle + area of the 3 segments outside the triangle.

The area of the triangle is $\frac{s^2\sqrt{3}}{4}$, that of the circle is πr^2; and if 2θ be the angle which the segment subtends at the centre of the circle, then area of segment $= r^2\,\theta - r^2 \sin\theta \cos\theta$, $= \frac{1}{2} r^2 (2\theta - \sin 2\theta)$.

But we have $\cos\theta = \frac{p}{r}$, if p is the perpendicular from the centre of the circle on the side of the triangle, viz. this is $\frac{1}{3}$ perpendicular from vertex on opposite side, $= \frac{1}{3} s \frac{\sqrt{3}}{2}$, $= \frac{s}{2\sqrt{3}}$, that is, $\cos\theta = \frac{1}{2\sqrt{3}} \frac{s}{r}$, and therefore $\sin\theta = \sqrt{1 - \frac{1}{12}\frac{s^2}{r^2}}$. Hence, in the former case,

$$\text{Aperture} = \frac{s^2\sqrt{3}}{4} - \pi r^2;$$

in the latter case,

$$\text{Aperture} = \frac{s^2\sqrt{3}}{4} - \pi r^2 + \frac{3}{2} r^2 \left\{ 2\cos^{-1}\frac{1}{2\sqrt{3}}\frac{s}{r} - \frac{1}{\sqrt{3}}\frac{s}{r}\sqrt{1 - \frac{1}{12}\frac{s^2}{r^2}} \right\};$$

or, what is the same thing,

$$= \frac{s^2\sqrt{3}}{4} - \pi r^2 + 3 r^2 \cos^{-1}\frac{1}{2\sqrt{3}}\frac{s}{r} - \frac{1}{4} s \sqrt{12 r^2 - s^2}.$$

[ED.]

Hyperbolical Elements of Comet I. 1845, *derived from* 244 *Observations. Osculation and Mean Eq.* 1845·0.
By Dr. W. Doberck.

T = 1845, Jan. 8, $4^h\ 1^m\ 41^s \pm 2^m\ 26^s$ m. Par. T.

	°	′	″	
π	91	19	55·9	±5·9
Ω	336	44	25·9	±0·9
i	46	51	0·8	±8·8
$\log q$	9·956 7491			±0·000 0055
e	1·000 2467			±0·000 1047

Direct.

Col. Cooper's Observatory,
1874, December.

Discovery and Observations of Minor Planet (139).

By J. C. Watson, Esq.

(*Communicated by the Astronomer Royal.*)

On the evening of Saturday, October 10, I noticed a star of bright eleventh magnitude in the following place:—

Peking M.T.			α			δ	
	h	m	h	m	s	°	′
Oct. 10	8	30	0	58	14·5	+10	43·5

Subsequent observations having indicated motion, and that the object observed is a new Planet, I proceeded to observe it as well as the means at my command would permit. The Equatoreal being provided with only a double-image micrometer of short range of measurement, I made use of occulting bars placed at right angles and inclined 45° to the parallel, and thus I have succeeded in obtaining the following observations of the Planet. The places of the Comparison-Stars were observed by Professor Young with our meridian transit instrument, the declinations being found by measuring with the zenith telescope, micrometer, and level, the difference of zenith distances north and south of the zenith between these stars and 38 *Cassiopeiæ*. The following are the observations of the Planet:—

	Peking M.T.				α			δ		
		h	m	s	h	m	s	°	′	″
1874	Oct. 10	13	30	0	0	58	1·68	+10	42	52·3
	11	9	58	15	0	57	15·48	10	40	33·9
	11	10	43	44	0	57	13·70	10	40	22·7
	11	11	31	0	0	57	11·75	10	40	24·8
	13	8	33	39	0	55	29·90	10	34	56·9
	13	9	43	44	0	55	27·67	10	34	38·5
	13	9	56	45	0	55	27·22	10	34	47·8
	14	9	35	11	0	54	33·71	10	31	57·3
	15	7	49	29	0	53	43·70	10	29	10·6
	16	8	58	34	0	52	46·46	10	26	15·0
	17	9	38	28	0	51	53·72	+10	23	8·5

Magnitude of the planet, 10·5.

Please communicate the foregoing observations to the Royal Astronomical Society.*

Peking, 1874, *October* 18.

* The planet discovered by M. Palisa on October 13, three days after the discovery of Mr. Watson's planet, must in future be designated as "Minor Planet (140)." See Note on its discovery, p. 64 of the *Monthly Notices* for November 1874.—[E. D.]

The Equestrian Equatoreal.

By Rev. E. L. Berthon, M.A., Vicar of Romsey, Hants.

The object aimed at in this new form of telescope is to bring a really good equatoreal mounting within reach of that increasing multitude of observers whose means prevent them from obtaining instruments of the usual form.

The method of obtaining an equatoreal motion is that of a turn-table, upon which the main tube is erected, with any number of supports or struts, so that perfect stiffness is gained with a great diminution of weight, and the effect of gravity upon a plane inclined to coincide with the co-latitude of the place is entirely neutralized by a pair of counterpoises on the other side of the plane.

Vibration in these telescopes is not perceptible; and when applied to Newtonian Reflectors they have great advantages, especially that of an unusual length of cradle in which the tube rotates.

A unique feature in these telescopes is that they are supported at their actual centre of gravity, owing to which, though they have an unusually broad base and large surface contact, friction is reduced to a minimum, so that a very small power is sufficient to drive them. The whole of the clockwork for a $12\frac{1}{2}$-inch telescope being contained in a little frame 5×3 inches, and the equalisation of its rate produced by a fan of peculiar construction.

This latter contrivance seems well adapted to many purposes connected with astronomical observations, as it requires no attention to position and is not affected by gravity. The fly, which regulates the speed, consists of two equal fans connected together by toothed wheels, which are controlled by a balance spring, so that any tendency in the clock to acceleration is met by an increased spread of the fans, whereby a greater degree of air-resistance is obtained. The clock-line passes down through the central pivot of the plane, and the weight descends inside the pedestal.

The largest telescope that has been made on this principle is one of the earliest and least perfect of this form, nearly 19 inches aperture; and more recent experience points out that this is the best as well as cheapest method for the mounting of instruments of extraordinary size.

Should it be desired at any moment to retard or accelerate one of these telescopes without touching the driving-clock, it can be done in the following way:

An air-bag of india-rubber pressed by a weight conveys a little stream of wind on either side of the fly at the option of the observer, who, by means of a very small air-cock, controls it as he pleases.

On Observations of the Satellites of Uranus. By A. Marth.

(*Communicated by W. Lassell, Esq.*)

I should like you to mention at the meeting of the Royal Astronomical Society, that during the present appearance of *Uranus* observers may again have a chance of contributing something towards the settlement of the question respecting Herschel's additional satellites, if they will take the trouble of making careful eye-drafts of the stars in the neighbourhood, and along the track of the planet on January 28 and March 2 and 3, 1875, and on preceding and following nights. The evidence of these sketches will not bear so directly on the question at issue as that of sketches made last winter on the nights, mentioned in my letter in the *Monthly Notices* for December 1873, would have done, yet it will be of some service, especially if it should turn out that the opportunities of last winter have been everywhere neglected.

If I had got a fair opportunity, I should have settled the question some years ago, and, I trust, in a satisfactory manner. But the attempts which I repeatedly made, had to be made in such an unsuitable atmosphere, and under such distressing circumstances, that I had to give them up as futile and hopeless. The Washington observers ought now to give their help, and to furnish the requisite evidence from the heavens, so that a critical summing up of the arguments pro and con, may ensure a satisfactory verdict.

It is a great pleasure, that at last, after a long interval, we have got again new series of observations of the satellites of the outer planets, without which the older series could not be made properly available. Should I be in a position to be able to afford the time, I shall not fail to publish in future regularly Ephemerides, so that observers may know beforehand what to look for. But for the purpose I must first get hold of the new observations, and the requisite time for correcting my old elements of the satellites' orbits.

Note on a Discussion relating to the Rejection of Discordant Observations. By E. J. Stone, M.A. F.R.S.

I am afraid that we are sometimes more sensitive to the tone of criticism of our work than to the tone of our criticism of the work of others. I can, therefore, only venture to ask anyone interested in the discussion which has taken place to read over, in order of publication, my original paper in the *Monthly Notices*, April 1868, to understand what I professed to do, and I believe did, in that paper; then Mr. Glaisher's animadversions upon that paper in the *Notices* for April 1873, and my reply: and I would call especial attention to the statement of my supposed

views, interpolated with free comments on page 398 of Mr. Glaisher's paper.

I should be most reluctant to enter upon mere verbal criticism; but if Mr. Glaisher does attach any importance to the statement towards the close of his short note in the *Monthly Notices* for March, that "I never held that the h of every observation was 'à priori' equally likely to have any value from o to ∞, as I took the usual result as a first approximation." I would remark that the point of the method, developed at considerable length in the *Notices* for April 1873, was to give to the h of every observation the value, whatever that might be, which belonged to it from the run of the numbers in a proposed case. Upon this point, to show that I have not misrepresented Mr. Glaisher, I give three extracts from his paper. On page 394 we read: "The law (of facility) with h determined separately for each observation, can be made to represent the facts with a near approximation to accuracy."

On page 395, speaking of the arithmetical mean, "instead of saying after the application of the rule, 'This is the final result,' I say, No! this is not good enough for a final result; but it is good enough to weight the observations from, and thence a better result is obtained and so on. Of course, if the first approximation is very nearly the best that the observations will give, the process of weighting will alter it very slightly." Again, on page 397, "An anomalous observation would obtain a very small weight, but never none at all, and this is as it should be." Other extracts might be given. It is true that Mr. Glaisher attempted to solve the question by successive approximations, but if the process had been properly conducted, the results obtained must have finally tended, without limit, to those obtained by any correct direct process. I cannot, therefore, see the force of the remark, that the arithmetical mean was taken as a first approximation. It might shorten or lengthen the work according as the arithmetical mean was, or was not, a close approximation to the final result. More than this it could not do. At least such is my opinion, and will, I venture to believe, be the opinion of mathematicians generally.

Royal Observatory, Cape of Good Hope,
1874, *June* 14.

The present number contains a Lithographic Plate of Drawings of Coggia's Comet, by Mr. Plummer, communicated by him to the Society at the November meeting.

ERRATUM.

Page 27, lines 4 to 7. *The sentence which follows should have been within marks of quotation, as part of the quotation from Sir W. Herschel*:—

Most likely this satellite also was seen among the supposed satellites south of the planet, March 27, 1794, where we find mention made 'of some others south at a good distance.' In that case this will make a second observation.